TABLE III.

Observations of the Moon, 1862 to 1884.

Mean Errors of Longitude. Uncorrected and Corrected for Error in Mean Time.

Year.	Errors of Longitude. (Hansen − Observed). *Uncorrected.*	*Corrected.*
	″	″
1862 Greenwich	− 2·829	−2·829
1863	− 1·606	−1·606 *
1864	+ 0·121	−0·814
1865	+ 1·271	−0·220
1866	+ 2·142	−0·217
1867	+ 3·480	+0·357
1868	+ 4·117	+0·280
1869	+ 4·277	−0·352
1870	+ 4·828	−0·657
1871	+ 6·955	+0·435
1872	+ 7·309	+0·097
1873	+ 8·239	+0·200
1874	+ 9·294	+0·561
1875	+ 9·867	+0·365
1876	+ 9·800	−0·509
1877	+ 9·234	−1·898
1878	+ 8·219	−3·603
1879	+ 9·631	−3·124
1880	+10·265	−3·245
1881	+10·622	−3·791
1882 Radcliffe	+12·927	−2·508
1883 ,,	+14·615	−1·547
1884 ,,	+14·645	−1·907

On Screw-wear as affecting the N.P.D. of the Cape Catalogue for 1880. By E. J. Stone, M.A., F.R.S.

The statement which Mr. Gill has made of the existence of serious systematic errors in the North Polar Distances of the Cape Catalogue for 1880, due to the wear of the screws of the Transit Circle between 1856 and 1879, is one which is easily made; but which it will be found much more difficult to prove than to make.

* Here change in the unit of time took place.

That two surfaces which rub must to some extent wear is true; but that the relative wear over the five or six threads which are in continuous use in Observatory work is sufficient to lead to serious errors in the results is a question which can only be settled by a direct appeal to facts.

Fortunately, whilst at the Cape, I did directly examine this question. It was, and still is, my rule to test, from time to time, the working of the screws, as a whole, by observations of a fixed collimating mark, or, better still, by Nadir Point Determinations at different parts of the screws. If it can be shown that the screws will allow a direction to be accurately determined at any of the threads used in the work, then it is needless, for all practical purposes, to discuss further questions of screw-wear; and if any errors due to screw-wear or defects in the screws affect the results, such experiments as I have described will clearly indicate their magnitude.

The results of observations of the Nadir at 0^r and 2^r made in 1875 are printed in the Cape Observations for 1875; but in 1877, in consequence of discussions which had taken place in England with regard to screw-wear, observations of the Nadir direction were made over the whole range of the threads of the screws which *could* possibly be used in making observations, including some that no careful observer would think of using from the mere feel of the working of the screw. The result was to show that, so long as the observations were confined to the five or six middle threads of the screws which must be employed in passing from one division of the circle to the next, and which were constantly in use, the errors due to defect of the screws of the Cape instrument were restricted within very small limits indeed; but that the employment of one or two threads outside this necessary range soon brought relative errors into existence. The curve, which Mr. Gill has given, is the reproduction of one which I constructed from these observations in 1877. It shows clearly the smallness of the errors so long as the central threads are alone used; and the extreme rapidity with which the errors increase as the readings are unduly extended. But an inspection of the observations shows that no continuous curve can accurately represent the observations, the fact to which they point is a *per saltum* change at or about 5^r. Whether this rapid increase of error be due to constraint or not, the fact of the rapid increase of relative error when these outside threads are engaged is undoubted. Such rapidity of increase of the errors when the extreme threads of the screw are brought into play shows that it would be useless to compare errors of the screws at different times from a few existing readings, unless it were perfectly certain that no changes whatever had been made in the adjustment of the index from which the revolutions are taken. It would be quite possible that the screws should be working without error from 0^r to 5^r at one time, and that no inconsiderable errors should arise from the use of the same readings at another if the

index has been adjusted relatively to the screws. As a matter of fact such adjustments are made from time to time. This probably explains why Mr. Gill has come to a different conclusion to myself with respect to the existence of strained readings when the screws were new. I have not the records before me, but I know that I carefully looked into the matter at the time; but as the strain becomes important only when extreme readings are used, it is impossible for me now to say whether such erroneous readings began at 0^r, or -1^r, or -2^r; all I assert is, that within the limits of range of screws occasionally, but, I presume, accidentally, used, there were some indications in 1856–1860 of exactly the same character as that shown by the curve resulting from the observations made in 1877. In exactly the same way there is visible wear in the screws of the Radcliffe Transit Circle, but experiments made over the threads in constant use give such results as those which follow, which include errors of screws and of observations:—

1884, July 10.

Reading of the Declination Micrometer.	Resulting Nadir Point.	Correction to Mean.
15·1	21S 26′ 4″28	+0″15
16·1	4·47	−0·04
17·05	4·64	−0·21
18·3	4·34	+0·09
19·1	4·48	−0·05
20·3	4·26	+0·17
21·1	4·27	+0·16
22·2	4·56	−0·13
23·3	4·37	+0·06
24·1	4·65	−0·22
25·2	4·44	−0·01

1^r of the micrometer = 32″·045.

Similar results have been obtained in other yearly examinations. The index has been adjusted, so that if negative readings be not taken, the errors are confined within the limits indicated by these observations, which are quantities within which I cannot answer for our work. The assistants are instructed, as they were at the Cape, to carefully avoid negative readings, and I can hardly understand how Mr. Gill can have persuaded himself that screws worn only to the extent indicated by the Cape Nadir observations in 1877 could have led to serious systematic errors in the catalogued results. That such is quite impossible can be seen by an inspection of his Tables VII. and VIII., when it is remembered that the observations for Nadir, runs, and of stars were confined within the limits adopted in the Cape work. But it is

quite possible to give a perfectly independent proof of the non-existence of any serious systematic errors due to screw-wear.

The Cape Transit Circle was brought into regular use in 1856; and during the years 1856 to 1860 a very considerable number of observations were made of the stars which have been regularly observed at Greenwich since Bradley's time. These observations were reduced whilst I was at the Cape, and are contained in the Cape Catalogue for 1860. The N.P.D. of the Cape Catalogue for 1860 can be assumed to be independent of any serious errors due to the wear of the screws which were at that time new. Since the proper motions of these stars can be determined with some approach to accuracy from observations perfectly independent of the Cape results, we can bring up the N.P.D.'s from 1860 to 1880 with some accuracy without in the slightest degree diminishing any systematic discordances which may exist between the N.P.D.'s of the two Cape Catalogues. I have therefore selected from the Cape Catalogues 1860 and 1880 all the stars whose N.P.D.'s are fixed by at least ten observations in the 1860 Catalogue, and three observations in the 1880 Catalogue, and which have the proper motions in N.P.D., referred to the adopted precession constant, determinable with some approach to accuracy from a comparison between Bradley's observations, or in two cases Piazzi's, and recent Greenwich observations. The N.P.D. of the Cape Catalogue for 1860 are brought up to 1880 with Peters' constant of precession and the adopted proper motions, and the differences taken between these results and those contained in the Cape Catalogue for 1880.

The differences will be affected by at least three independent sources of error.

(1) There will be the error of the N.P.D. given in the Cape Catalogue 1860; I shall certainly not over-estimate the probable error due to this cause by taking

$$e_{1860} = 0''.1.$$

(2) There will be the combined effects of error in the precession constant and adopted proper motion for twenty years. As this cannot be less than $0''.1$, and will be about one-sixth of that due to the relative errors of Bradley's N.P.D. and the recent Greenwich observations, I cannot over estimate the probable error by taking

$$e_m = 0''.2.$$

(3) There will be the probable error of the N.P.D. of the Cape Catalogue of 1880. As there are generally only three or four observations employed in fixing the N.P.D., the probable error, independently of any possible error due to screw wear, will be about

$$e_{1880} = 0''.25.$$

We shall therefore have for the probable error of the computed differences 1860—1880 on the supposition that there are no systematic errors due to screw-wear

$$\overline{pe} = \sqrt{0\cdot1^2 + 0\cdot2^2 + 0\cdot25^2} = 0''\cdot33.$$

But as these screw-errors, if they exist, must increase the probable error due to mere ordinary chance errors of observing, we shall have, when the probable error due to the screw-wear is taken $=x$

$$\overline{pe} = \sqrt{0\cdot1^2 + 0\cdot2^2 + 0\cdot25^2 + x^2}.$$

If, therefore, x could be taken $= 1''$, we should have

$$\overline{pe} = 1''\cdot055.$$

If x could be taken $= 0''\cdot5$

$$\overline{pe} = 0''\cdot60.$$

If Mr. Gill does not mean to assert that the Cape N.P.D's are affected by error to $0''\cdot5$ from the cause indicated, then I think his paper should not have been written: but even if x could be taken at $0''\cdot25$ we should still have

$$\overline{pe} = 0''\cdot42.$$

Now a direct computation of the probable error from the observed differences gives

$$\overline{pe} = 0''\cdot28.$$

This would show that x is really insensible, and that no serious errors can have affected the N.P.D. observations contained in the Cape Catalogue for 1880 due to any such cause as screw-wear.

I append the comparisons between the two Catalogues from which the probable error above given has been deduced.

The agreement between the results, allowing for the few observations by which the N.P.D. of the 1880 Catalogue were fixed, is exceedingly good. There are only two cases out of the 92 comparisons in which the difference between the two Catalogues amounts to a second of arc. The mean excess of the N.P.D. of the 1880 Catalogue is only $+0''\cdot06$, the range of N.P.D. being between 51° and 123°, whilst the co-latitude determined from the observations 1856–1860 was

$$123^\circ\ 56'\ 3''\cdot56,$$

that found from the more recent observations and included in the Cape Catalogue 1880, was

$$123^\circ\ 56'\ 3''\cdot41.$$

I have in the Introduction to the 1880 Catalogue given comparisons between the Cape and Greenwich results, the mean discordance being 0″·31.

There does not, therefore, appear any such indication of constant error as might be inferred from Mr. Gill's remarks.

I hope that Mr. Gill will be better advised than to apply empirical corrections to the North Polar Distances of the Cape Catalogue for 1880. It is quite impossible for him to obtain the true corrections due to any errors which may have existed. The runs are an essential part of the reductions. If the screws had unfortunately been sufficiently worn to prevent reliable readings being taken at any part of the threads in regular use, then most certainly the threads in use for the run determinations could not have been trusted. But as proved from the experiments made during the progress of the work, the effects of screw-wear are so small that the systematic errors due to this cause are certainly less than the probable errors of mere observations in the catalogued results.

Name of Star.	Proper Motion in N.P.D.	Residual Errors N.P.D.		No. of Obs. in Cape Cat. 1860.	No. of Obs. in Cape Cat. 1880.
		−	+		
	″	″	″		
γ Pegasi	+0·011	−0·41		33	7
12 Ceti	+0·012		+0·48	13	9
β Ceti	−0·025	−0·10		122	3
δ Piscium	+0·048		+0·01	12	3
ε Piscium	−0·023		+0·11	34	5
θ Ceti	+0·220		+0·39	42	4
η Piscium	+0·007		+0·43	24	5
ν Piscium	−0·002		+0·03	16	4
β Arietis	+0·118		+0·25	30	3
α Arietis	+0·151	−0·24		38	5
67 Ceti	+0·110		+0·02	28	4
ξ^2 Ceti	+0·016		+0 32	13	4
α Ceti	+0·086	−0·16		31	5
δ Arietis	+0·002		+0·36	33	3
γ′ Eridani	+0·096		+0·37	30	4
o′ Eridani	−0·080		+0·94	29	3
α Tauri	+0·192	−0·06		103	4
ε Leporis	+0·072	−0·16		26	7
β Orionis	+0·006		+0·09	109	5
β Tauri	+0 181	−0·29		35	3
δ Orionis	+0·005		+0·14	35	5
α Columbæ	+0·045	−0·21		39	7

Name of Star.	Proper Motion in N.P.D.	Residual Errors N.P.D. −	Residual Errors N.P.D. +	No. of Obs. in Cape Cat. 1880.	No. of Obs. in Cape Cat. 1830.
	″	″	″		
α Orionis	−0·007	−0·17		90	3
ζ Canis Maj.	+0·005		+0·16	10	3
γ Geminor.	+0·048	−0·13		17	3
ε Canis Maj.	+0·013	−0·32		70	4
δ Geminor.	+0·016		+0·52	31	3
6 Cancri	+0·036		+0·79	11	6
15 Argûs	−0·059	−0·42		49	6
η Cancri	+0·054		+0·35	27	3
ε Hydræ	+0·053	−0·09		19	3
α Hydræ	−0·030		+0·27	67	6
π Leonis	+0·022		+0·65	27	5
α Leonis	−0·012	−0·31		88	3
χ Leonis	+0·043	−1·10		43	3
δ Crateris	−0·183	−0·19		71	3
τ Leonis	+0·021		+0·73	16	3
υ Leonis	−0·035		+0·16	22	3
β Leonis	+0·119	−0·21		14	3
ε Corvi	−0·020	−0·36		19	3
β Corvi	+0·066	−0·30		98	3
θ Virginis	+0·041		+0·54	26	7
α Virginis	+0·038		+0·35	118	4
ζ Virginis	−0·059		+0·80	11	4
α Boötis	+1·977	−0·27		37	4
20 Libræ	+0·032		+0·04	15	3
β Libræ	+0·031		+0·35	42	3
α Serpentis	−0·034	−0·26		17	3
β Scorpii	+0·038		+0·01	77	4
δ Ophiuchi	+0·143		+0·49	24	4
σ Scorpii	+0·026	−0·47		14	3
α Scorpii	+0·037	−0·45		139	13
τ Scorpii	+0·043	−0·63		20	3
θ Ophiuchi	+0·029	−0·52		53	4
α Ophiuchi	+0·218	−0·34		20	3
Stone 9795	+0·039		+0·22	11	3
μ Sagittarii	+0·012	−0·36		72	3
δ Sagittarii	+0·027		+0·14	29	3
ε Sagittarii	+0·130		+0·61	19	4
λ Sagittarii	+0·224	−0·36		20	5

Name of Star.	Proper Motion in N.P.D.	Residual Errors N.P.D. −	Residual Errors N.P.D. +	No. of Obs. in Cape Cat. 1860.	No of Obs. in Cape Cat. 1880.
	"	"	"		
α Lyræ	−0·272		+0·06	16	3
φ Sagittarii	+0·008	−0·53		24	3
σ Sagittarii	+0·077	−0·02		40	3
ζ Sagittarii	+0·006	−0·49		23	3
τ Sagittarii	+0·251	−0·08		18	3
ψ Sagittarii	+0·029	−0·28		14	4
ω Aquilæ	−0·022		+0·41	24	3
δ Aquilæ	−0·076	−0·02		36	5
h^2 Sagittarii	+0·027		+0·19	46	3
γ Aquilæ	+0·008		+0·13	12	4
α Aquilæ	−0·380	−0·95		108	5
b Sagittarii	+0·036		+0·05	13	3
c Sagittarii	−0·016	−0·26		21	5
α^2 Capricorni	0·000		+0·85	86	3
ρ Capricorni	+0·020	−0·08		46	4
ψ Capricorni	+0·162	−0·16		15	4
θ Capricorni	+0·075		+0·39	12	3
ι Capricorni	−0·004		+0·41	16	3
β Aquarii	+0·016		+0·27	95	7
ε Pegasi	+0·006		+0·33	21	3
δ Capricorni	+0·313		+0·13	21	3
α Aquarii	+0·014	−0·20		33	10
θ Aquarii	+0·025		+0·62	35	3
σ Aquarii	+0·030		+1·08	12	5
η Aquarii	+0·040		+0·27	22	7
γ Piscis Aust.	(0·000)		(+0·01)	10	3
α Piscis Aust.	+0·171		+0·26	123	6
φ Aquarii	+0·177		+0·33	10	3
γ Piscium	−0·009		+0·31	26	3
κ Piscium	+0·111	−0·08		36	9
ι Piscium	+0·443		+0·66	23	9
δ Sculptoris	+0·103		+0·02	16	3
ω Piscium	+0·104	−0·20		27	7

On account of the variability in their proper motions I have not included *Sirius* and *Procyon*.

It is perhaps desirable that some notice should be taken of the statement which Mr. Gill has made in the Introduction to the Cape Catalogue for 1850 with respect to the differences

between the Right Ascensions of this Catalogue and those of the Cape Catalogue for 1880 when reduced to a common epoch. Mr. Gill appears to consider that these differences indicate systematic errors in the Cape Catalogue for 1880 due to variation in the place of collimation. I must remark, however, that materials do not exist for an accurate determination of the proper motions of the southern stars with reference to the adopted precession constant; and we cannot bring up places for a period of thirty, or even twenty, years with sufficient accuracy to justify attempts to base discussions of instrumental systematic errors upon differences of observed places when thus compared. Such differences may, of course, be due to instrumental defects, but they may be principally due to uncorrected proper motion. But the mean difference found by Mr. Gill of $0^s.06$ divided by 30 years gives only a yearly change of $0^s.002$, and this is within the possible, and I should suppose probable, errors of the determination of our precession constant. I have compared the Melbourne Right Ascensions made in 1875 with the Cape Observations 1870–1880; and in this comparison we are not much concerned with proper motions. The result, which I find appears clearly to indicate that the differences between the Cape Catalogues 1850 and 1880, are not due to either lateral refraction or any systematic variation in the plane of collimation of the Cape Transit Circle, unless the Melbourne instrument is affected by sensibly equal errors. The Melbourne clock errors are apparently based on the Greenwich standard places, and as the Cape results are also based on the same places, the Right Ascensions of the clock stars must therefore agree, and I have not included their differences in the following result. But between 124° and 170° N.P.D. I have found 46 stars which were observed at Melbourne in 1875, and whose Right Ascensions are contained in the Cape Catalogue 1880. Instead of finding the Cape R.A. too small, as would appear to be the case from Mr. Gill's comparisons with the Cape Catalogue for 1850, I find the difference reaches the limit of two-tenths only in one case, when a single observation only was used for Melbourne R.A., and that the mean excess of the R.A. Cape 1880, over R.A. Melbourne 1875, is only $+0^s.02$. I do not wish to assert that the Cape results are free from systematic errors, but I am certain that if such exist they are small, and that such systematic errors in Right Ascension as may exist will be chiefly dependent upon the adopted Right Ascensions of the clock stars.

Addendum.

There are some statements of Mr. Gill's on pages 80 and 81 to which, on seeing his paper in print, it appears desirable that I should make some reply.

1. The screw constraint was not due to dirt. It was present, whether the screws were clean and fresh oiled or not, when certain threads were brought into play.

2. "Occasional notes such as 'screws turned stiffly,' followed soon after by a note that 'the screws were cleaned,' and there are no more notes about stiffness of the screws for a long time," merely show that the Cape screws, like other screws, occasionally required cleaning, and were cleaned.

3. Mr. Gill's attempted proof of progressive wear between 1856–57 and 1872–73 consists merely in showing that the runs were about $0^{r}.014$ greater in 1872–73 than in 1856–57, and an *assumption* that there were no adjustments of the microscopes between those dates. If Mr. Gill had given the runs after June 1873, he could have proved, on the same assumptions, that the screws had worn in the opposite direction; for the runs after June 1873 are about $0^{r}.028$ *less* than those in 1857–58. Mr. Gill must have seen that such changes are principally due to adjustments; and he might have known that as the microscopes A and B had to be taken off in 1873 to allow the supplementary microscopes *a* and *b* to be dismounted, as is stated in the Cape Results, 1873, page xvi.; and as these microscopes *a* and *b* had been mounted by me soon after my arrival at the Cape, 1870–71, in order to determine the division-errors of the circle, the microscopes A and B must, at that time, have also been dismounted in order to mount the microscopes *a* and *b*. If, therefore, we suppose that the adjustments of the microscopes had not been disturbed after 1857–58 till I arrived at the Cape, a most improbable assumption, they certainly had been disturbed before the readings 1872–73.

4. An inspection of the observations made by me in 1877 shows clearly enough that no continuous curve can be drawn to represent the errors of the screws; the observations between $5^{r}.1$ and $5^{r}.8$ fall on a straight line, and there is less discordance than $0''.5$ between $5^{r}.1$ and $10^{r}.1$. The fact brought out by these observations is that the errors changed *per saltum* when the parts of the screws which corresponded to the readings about my 0^{r}, or Mr. Gill's 5^{r}, were brought into play. If, therefore, it was wished to make accurate observations with these screws, it could be done by avoiding the use of these threads about 5^{r}, and in no other way, and this is the course which I have followed. This is clearly shown by Mr. Gill's extracts from my notes, for it will be seen that even in 1872–73 negative readings were most carefully avoided. The indices of the microscopes were adjusted so that the defective threads of the screws could not be employed in the observations so long as negative readings were avoided; and the assistants were cautioned against the use of negative readings. If I found that such readings for runs were accidentally made, they were no doubt rejected because it was known that they must be defective, and ought not to have been made; and if at any time I had reason to suppose that the defective threads were from slight changes again coming into use, the indices were readjusted to avoid the further use of these defective threads. An inspection of the results of the

observations made in 1877 shows that with these precautions the effects of screw errors must have been confined within very small limits even in individual observations, and in the results of three or four observations must be practically insensible, and this result is confirmed by the comparison which I have given between the Cape Catalogue 1860 and the Cape Catalogue 1880. The larger errors which appear in Mr. Gill's Tables VII. and VIII. have nothing to do with the systematic errors of the Cape Catalogue 1880, for the simple reason that the threads of the screws to which they apply were not used in the work.

Mr. Gill's idea appears to be that I was bound to make observations over the whole range of the screws, whether the threads were defective or the screws under constraint or not, and then to attempt to determine the errors of these screws and to apply corrections to the results. I preferred to avoid the large errors altogether, and I have done so. Every attempt to carry out Mr. Gill's ideas into practice with the Cape screws would have ended in a disastrous failure. The change of error is so rapid about the 0^r (or Mr. Gill's 5^r) that the errors of the screws could not be determined with any great accuracy, or applied with any certainty subject to such shifts of adjustments as are inevitable in practical work. But whether Mr. Gill's ideas might have been practically carried out or not, I was certainly entitled to carry out my own ideas, which I knew would lead to results of very considerable accuracy. I have carried them out, and I am satisfied with the result.

Note on the Descriptions of two Stars in Ptolemy's Catalogue.
By E. B. Knobel.

In the first printed edition of the *Almagest*, which is that published in Latin by Liechtenstein at Venice in 1515, the descriptions of the 8th star in *Scorpio* (*α Scorpii*) and the 2nd star in *Orion* (*α Orionis*) are as follows:—

8. *Scorpio.* "Media earum quæ *tendit ad rapinam* quæ dicitur Cor Scorpionis."
2. *Orion.* "Lucida quæ est super humerum dextrum et ipsa *tendit ad rapinam* quia * appropinquat ad terram in humero Orionis."

Baily remarks in his compiled edition of *Ptolemy's Catalogue*, "There is a singular expression in the edition of Liechtenstein that I am unable to explain. It first occurs in the constellation *Scorpio*, and is repeated in the constellation *Orion*, where the star is described as "tendens ad rapinam."

It is well known that the Liechtenstein *Almagest* is derived

* Baily translates the abbreviation in Liechtenstein as "quæ," but Mr. Scott, Assistant-Keeper of MSS. British Museum, tells me it is undoubtedly "quia."